part of the night between the 14th and 15th, as well as throughout the night between the 15th and 16th of this month, beginning the watch at 10.30 p.m. Before that hour observations cannot be made, the radiant point of the Leonids being below our horizon.

A computation of the perturbations of two other stations in the stream, in which we have made use of Dr. Berberich's results to supplement our own, has established a fact which we had anticipated, viz. that different parts of the immensely long ortho-stream have been so variously affected by perturbations that the stream cannot now be a uniform one evenly extended along a portion of its elliptic orbit. We must accordingly recognise that it is more or less sinuous, and that, moreover, the distribution of meteors along it is uneven. All such circumstances introduce further elements of uncertainty into any attempt which we may make to form a forecast.

On account of the abnormal amount of the perturbations within the last thirty-three years, the method by which the prediction was made in 1866 is not sufficient on the present occasion. It was based on the average amount of the shift of the node. If employed on this occasion, it would assign for the shower of this year the epoch 1899 November 14d. 12h., which is almost certainly too early.

London, November 4. G. JOHNSTONE STONEY.
A. M. W. DOWNING.

Undercurrents.

I AM much obliged to Admiral Makaroff for so courteously answering my queries in my letter to NATURE in the number for August 3, p. 316; and I regret that having mislaid my copy of his book, the "Vitiaz," I have not been before able to reply. On now studying his observations in the Strait of Bab-el-Mandeb, and his remarks in his letter (NATURE, October 5, p. 544), I fear I cannot any more share his opinions than I did before.

My point is that it is not sufficient to ascertain that there is a difference of specific gravity either between the surface water or either side of a strait, or between the surface and lower strata of water in a strait, to be able to come to the conclusion that such difference is the primary cause of surface and undercurrents in opposite directions. It has been shown by experiment that such differences give rise to a slow interchange of water, and to this extent I am of course prepared to agree that differences of specific gravity cause opposing currents; but the currents we are dealing with are of a vastly different character and strength.

I have already pointed out that my observations in the Dardanelles and Bosporus in 1872 showed that the currents did not always run in the normal directions, and that their variations were traceable to the varying winds; and to that I have nothing to add.

On looking at Admiral Makaroff's density observations in the Strait of Bab-el-Mandeb, I see that the specific gravity varied from 1·0279 at the surface to 1·0292 at 200 metres, while the surface waters of the Red Sea itself and of the Arabian Sea near Sokotra are given as 1·0300 and 1·0279 respectively.

In saying that "here are none of the differences of specific gravity demanded by Admiral Makaroff's hypothesis," I was referring to the great contrast between the difference in the densities of the Black Sea and Mediterranean, viz. ·017, and those of the Arabian Sea and Red Sea, viz. ·002, and never thought for a moment that the very small variation in density in the latter case could be held capable of setting up currents of $1\frac{1}{2}$ knots in opposite directions at surface and bottom, as found by "Stork's" observations in the N.E. monsoon.

No observations have yet been made on the undercurrent in Bab-el-Mandeb in the S.W. monsoon; but the surface current is known to run in the contrary direction to what it does in the N.E. monsoon, *i.e.* again with the wind, or *out* of the Red Sea, and I should be much surprised to find that the undercurrent does not also run in the contrary direction, probably with much greater strength than the surface current, because the great evaporation of the sea has also to be made up.

Absolute proof of the causes of such phenomena as these under discussion comes slowly, and only after laborious observation; but I certainly think that the work of the last twenty-five years has tended to show that the influence of density as compared with wind is insignificant. W. J. L. WHARTON.

Florys, Wimbledon Park, November 4.

"Anlage" and "Rudiment."

SOME months ago Prof. Herrick, who is in charge of the department of Neurology in the "Dictionary of Philosophy and Psychology," which I am editing (now in the press of Macmillan and Co.), addressed a circular to various authorities asking their opinion on certain matters of terminology. The results were collated and discussed by Prof. Herrick in the *Journ. of Comp. Neurology*, vii. 3–4, 1898. Among the matters in question was the English equivalent of the German term *Anlage*. Prof. Herrick came to the conclusion that *Proton* and *Rudiment* were more available than any other words suggested (ruling out the use in English of *Anlage* with its German inflexion).

It now happens that the French and Italian committees, who are recommending equivalents, in their respective languages, for the terms in the Dictionary, make reports which I think are of importance. Prof. Delage, of Paris, for the French committee, recommends *rudiment*, and, as it happens, Prof. Morselli, of Genoa, sends in *rudimento* as the preferred Italian term. This agreement—and to say this is my aim in writing—affords a strong argument for the adoption of *Rudiment* in English. It is evident that it would be of immediate and very great advantage—for example, to translators from any one of these languages into any other—if *Rudiment* were made the common rendering of *Anlage* in the three other languages of modern science. The other great advantage would be that we already have the adjective form, *rudimentary*, in use.

Furthermore, the psychologists may use the same term for the German *psychologische Anlage* which has crept into recent German discussions. In English, biologists and psychologists will then have the common term *rudiment* with a well-understood signification. I am recommending this to the committee on terminology of the American Psychological Association, of which I happen to be secretary. J. MARK BALDWIN.

Oxford, October 22.

Interference Curves depending on Perspective.

CLOSELY allied to the halo round an observer's shadow (referred to in Mr. S. Newcomb's letter in NATURE, October 5) are a number of phenomena due to perspective, which may be seen every day by any one who is on the look out for them.

Among these may be mentioned the dark waves which seem to accompany a traveller when he looks through two series of upright palings which lie parallel to each other and his course; also the patterns like the grain of wood which appear when two superposed sheets of gauze are held against the light.

As these and the like appearances have not, as far as I know, hitherto been looked at from a mathematical point of view except in one instance,[1] the three following examples, which are typical but simple, may be of interest:—

(1) Interference rings due to parallel lines on a spherical surface, and their shadow or reflection on a plane or in a plane mirror.

Let a small part of a sphere of radius r be ruled with equidistant parallel lines, the distance between the lines being small compared with r. Let the convex surface of the sphere touch a plane mirror, and let surface be viewed from a distant point, the line joining the distant point, and the point of contact making an angle i, with the normal to the plane.

Taking the plane containing the point of view and the normal through the point of contact as the plane of reference, let a be the angle between the plane containing a line and its reflection and the plane of reference. Let θ be the angle which a point on the spherical surface distant ρ from the normal through the point of contact subtends at the centre of the sphere (so that $r \sin\theta = \rho$), and ϕ the angle which ρ makes with the plane of reference.

It is plain that where, from the point of observation, any part of any line hides the reflection either of itself or any other line, the field will look brighter in that direction than where the line and the reflection are both visible, and the condition which must be fulfilled in order that one line may hide the reflection of another n lines off is (neglecting second order quantities) that the distance between the hidden reflection and the line reflected (that is, twice the distance of the line from the reflector) multiplied by tan i should be equal to n times the projection of a on the plane of reference, or in symbols,

$$2r(1-\cos\theta)\tan i = \frac{na}{\cos\alpha},$$

[1] Lord Rayleigh's "Theory and Manufacture of Diffraction Gratings" (*Phil. Mag.*, 1876).

since this equation does not contain ϕ, ρ is constant whatever the value of ϕ may be; hence the bright parts of the field are circul ır rings surrounding point of contact of the plane and spherical surface.

The radii of the dark rings can be deduced from the relation

$$2r(1-\cos\theta)\tan i=\frac{2n-1}{2}\,\frac{a}{\cos\alpha};$$

and from these equations it is easily shown that for the bright rings

$$\rho=\sqrt{\frac{\imath an}{\tan i\cos\alpha}},$$

and for dark rings

$$\rho=\sqrt{\frac{\frac{2n-1}{2}}{\tan i\cos\alpha}}.$$

This may be compared with the corresponding values for Newton's rings.

In both cases the radii of the bright and dark rings vary as the square roots of the even and odd numbers, and as the square root of the radius of the sphere and the wave length (which is analogous to a in the present case), but here the likeness ceases.

The rings here considered diminish as i increases, and increase as α diminishes.

Of course, in Newton's rings there is nothing which answers to the angle α.

The easiest way of examining these rings is to mould a small circle of wire gauze to form part of a sphere (which can readily be done by pressing it with a ball against any yielding substance) and laying it, convex surface downwards, on a piece of looking glass.

In general two sets of rings will be seen, one due to the wires of the warp, and the other of the woof of the gauze.

When the eye. however, looks parallel to one set of wires, the rings of that set are all infinite, and only the set due to the wires at right angles to the line of sight are visible.

If the gauze is made to turn slowly on the point of contact, both series appears, one growing, and the other diminishing, which are exactly superposed when $\alpha = 45°$.

A curious effect may be observed when a thick plate of glass is placed between the gauze and the looking glass.

The rings in this case become coloured, showing blue on their inner, and red on the outer margins of the dark bands.

The explanation is obvious, for the pencils of white light entering through the meshes of the gauze are dispersed on entering the glass, and in the neighbourhood of the dark rings only part of the dispersed pencils are cut off on their second passage through the gauze, so that the light which reaches the eye is coloured.

If t is the thickness of the glass plate, the greatest colour effect is obtained when $t=b\mu/2d\mu\tan r$, where b is the diameter of the wire and r the angle of refraction in the glass.

When the glass plate is used, of course the smallest visible ring is not that for which $n=1$, but it is unnecessary here to enter on the alteration in the formula for ρ caused by putting $2r(1-\cos\theta)+t)$ for $2r(1-\cos\theta)$.

(2) Interference rings caused by two series of straight lines, radiating at equal angles, from two centres in the same or parallel planes.

Let there be n lines in each series, then the angle between successive lines in each series is $2\pi/n$.

Let the lines of the first series be numbered $1, 2, 3 \ldots p \ldots n$ and those of the second $1', 2', 3' \ldots q' \ldots n$, and let the line 1 be parallel to the line $1'$. Then the angle made by any line p of the first series with another q' of the second is $(p-q)2\pi/n$, hence the intersections of all pairs of lines for which $(p-q)$ is the same will lie on a circle passing through the two centres and having this segmental angle.

When the distance between the centres is a, the radius of the circle is

$$\rho=a\operatorname{cosec}2\pi(p-q)/n$$

if both centres are in the same plane, or

$$(a+b\sin i)\operatorname{cosec}2\pi(p-q)/n$$

if in different planes, where a = distance between the normals to the planes through the centres, b the distance between the planes, and i the angle made by the line of sight with the normal.

The loci of the intersections appear brighter than any other part of the field of view, hence the intersections of the two series show as a family of bright and dark circles which all pass through the two centres, and whose radii are as the cosecants of the multiples of $\frac{2\pi}{n}$.

This is shown in Fig. 1.

A pair of wheels of a carriage, one viewed through the other, show the phenomenon very well, especially when the wheels are turning fast enough to make the individual spokes indistinct.

Under favourable circumstances as to light and background, the appearance of the rings, contracting and expanding as the angle of view changes, is very striking.

(3) Interference curves from two series of straight lines, one radiating and the other parallel.

From a point P in the axis of Y let radiating lines be drawn to cut the axis of X at equal intervals a, and at $a, 2a, 3a$, &c., let lines be drawn parallel to Y.

Then, if the distance of P from the origin is h, to determine the coordinates of the intersection of the nth parallel line with the $n+p$th radiating line, we have, since $x=na$,

$$\frac{h-y}{h}(n+p)\,a=na,$$

hence

$$\frac{y}{h}(n+p)=p.$$

The locus, therefore, obtained by giving the value 0, 1, 2 .. ∝ to n will be a series of points on a rectangular hyperbola passing through P with its centre at $y=o$ $x=-pa$.

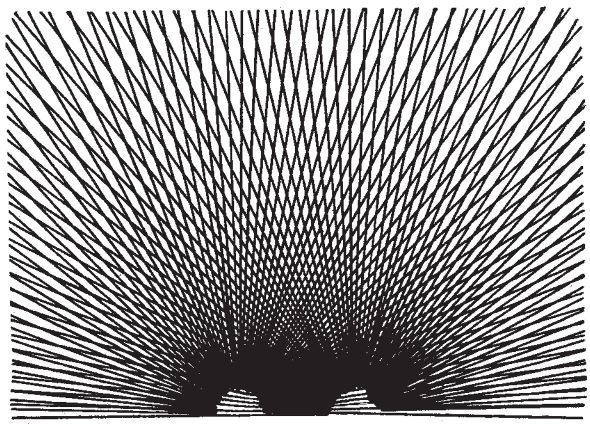


FIG. 1.

Thus the field of view will show a family of hyperlolæ (one for each value of p), all passing through P, the parameters being $\sqrt{pha}$.

In the same way, for the intersections of the nth parallel with the $2n+p$th radiating line we have

$$\frac{y}{n}(2n+p)=p,$$

which indicates a second fami'y of hyperbolæ, the coordinates of the centres being $\frac{h}{2}$ and $-\frac{p}{2}$.

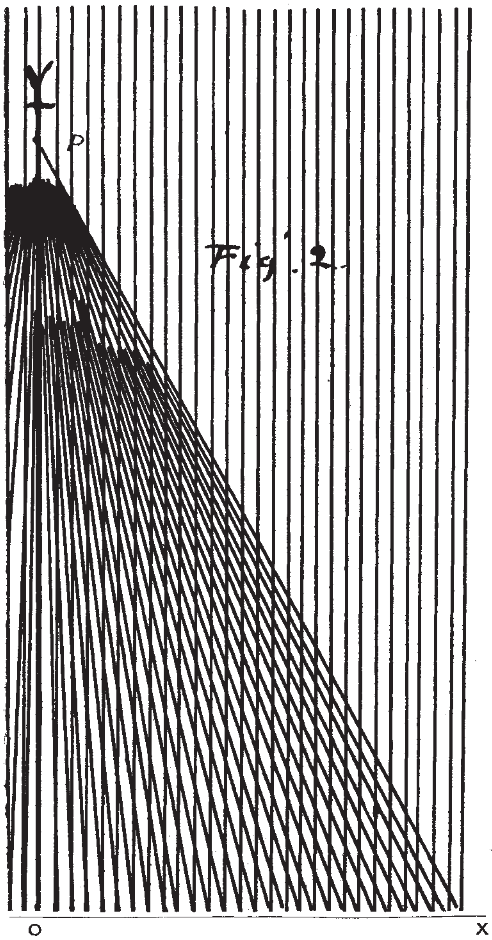
Similar families are formed by the intersection of the nth parallel, with the $3n+p$th . . . $4n+p$th . . . &c., radiating lines, the corresponding centres being $y=\frac{2h}{3}, \ldots \frac{3h}{4}$, &c. and $x=-\frac{p}{3}, -\frac{p}{4}$, &c.

It will be readily seen that the dark and bright bands formed by the interfering lines follow the short diagonals of the quadrilaterals into which the two series of lines divide the field, and that for the bands to be conspicuous, there should be a great difference in the length of the two diagonals, and only a small difference in the length of the sides of the quadrilaterals.

For this reason only a part of each hyperbolic family is recognisable.

In Fig. 2 the first two families are both well seen in parts, and some of the curves of the third family can be recognised.

We may, if we choose, consider the radiating lines as the perspective view of a series of parallel lines in a plane inclined to X Y.



We then have the case of a row of vertical railings and their shadows on the ground.

In passing a line of such railings when the sun is low, the curves, which appear to travel with the observer, may often be noticed. A. MALLOCK.

3 Victoria Street, S.W., October 16.

The Indian Forest Service.

I AM very glad to see Prof. Schlich's defence of the Indian Forest Service in NATURE of November 2. I have myself been very closely connected with forest administration in the Bombay Presidency from 1871 to 1894. I may say that I have seen the Department there grow up from little better than chaos into a well-organised corps of spirited and well-trained officers; and there is not one word in Prof. Schlich's letter that I cannot heartily support.

Botanists can hardly be too abundant in India; but if we want good systematic botanists there, we must call them by that name, and either train them specially to that science, or get men so trained in the market.

It is to me surprising that the Indian forest officers have done so much botanical work as they have, to say nothing of the services of several of them to zoology; and it must be remembered that their appointments are even now won at a considerable cost in toil and money, that their pay is not high, and that their duties involve at least as much hard work of body and mind, as much hardship, and as much risk, as those of any other service in India.

Forestry is not all botany. It may, perhaps, be best defined as the "proper management of hardy life upon large areas." And the man who does that best is the best forester. We have a great many who do it well, and amongst these there will always be some to whom systematic botany is labour of love. But to insist upon any great general proficiency in one of the many subjects that a forester must study, will simply injure the general efficiency of the forest corps; and probably fail in the case of the favoured subject. W. F. SINCLAIR.

November 3.

Peripatus in the Malay Peninsula.

IN a recent issue of NATURE (October 19) the interesting fact is mentioned of the discovery by the Skeat Expedition of several specimens of Peripatus in the Malay Peninsula. Will you allow me to remark that in 1886 I described in the Notes from the Leyden Museum a specimen of Peripatus from East Sumatra, found among a lot of insects collected by Mr. Hekmeyer, of our Indian Medical Service. As it was the first specimen recorded from the Oriental region, Prof. Sedgwick, in his elaborate monograph of the genus Peripatus, considered the Sumatra species to be somewhat doubtful. The specimens, however, found by Mr. Evans in Kalantan appear to correspond so well with our specimen, as well in the number of pairs of feet (24) as in the colour, that I think a more detailed account will prove the identity of the animals found on both sides of the Malaka Strait. R. HORST.

Leyden Museum of Natural History, October 30.

A Wooden Ball of Unknown Origin.

ON the shore of the island of Hadod, latitude 68° 40′ about, in Vesteraalin, north of Lofoten, there was found, probably in the autumn of 1897, a wooden ball, $4\frac{1}{2}$ centimetres in diameter, covered by a thin layer of gum. The ball is of fine workmanship, and just able to float in the water. Circles are engraved upon four parts, and form small rhumbs over the whole surface; and on two places there is engraved with Latin Majuscles the name *Melfort*. Perhaps some of your readers can say from whence this ball has come. I am writing to the man who has the ball now, to ask him to send it to me. H. MOHN.

Det Norske Meteorologiske Institut, Kristiania, October 30.

Large Nicol Prisms.

IN the account of Dr. Spottiswoode's physical apparatus, lately given to the Royal Institution, there are allusions to several large Nicol prisms said to have been made by Mr. Ladd and by Messrs. Sisley and Spiller. Although it is no doubt the trade custom to mention only the names of opticians who sell pieces of apparatus, and not of any of those whom they employ to make them, I still venture to hope that in this case, where skill and labour of a very special kind were required, the name of the actual maker of the above-mentioned prisms may not be forgotten. I would therefore respectfully ask permission to give a few particulars as to size, &c., of some of the larger Nicol prisms which I have myself made from blocks of Iceland spar within the last thirty years.

(1) In 1873 Dr. Spottiswoode bought a very fine block of spar from Mr. Tulinius, of Copenhagen (who then owned and worked the spar quarry at Eskifiördkr in Iceland). Out of this, which was absolutely flawless, I made a Nicol prism having a clear field of $3\frac{5}{8}$ inches diameter, the length of each side being 12 inches.

(2) In 1874 I made a second prism from the same block of spar just mentioned, and also a third from another piece of spar bought by Dr. Spottiswoode. Both of these prisms had a clear field of $3\frac{1}{2}$ inches, the length of the sides being $11\frac{1}{2}$ inches. These are now at the Royal Institution.

(3) In 1875 I made a Nicol prism for Mr. Frank Crisp, of $3\frac{1}{2}$ inches field and $11\frac{1}{2}$ inches in length, which he used in a polariscope in conjunction with the first one mentioned above, which he had acquired from Dr. Spottiswoode. These Mr. Crisp sold, and are now in England.

(4) In 1876 I made two more large prisms for Dr. Spottiswoode, one of 3-inch and the other of $2\frac{1}{2}$-inch field, as spar was